or even in the same individual. The intensity of the excitation has an important function ; the rate of transmission augmenting with it, especially in sensory nerves. For the motor nerves, when the excitation becomes strong, the rapidity diminishes. In locomotor ataxia there is a considerable retardation of rapidity of transmission of nerve-force, whilst in the beginning of general paralysis the rate is more rapid than normal.—*Gazette des hôpitaux*, 1882, No. 46.

Motor Centres.—Brown-Séquard has made a large number of experiments which, to him, appear to demonstrate that the existence of motor centres in the brain and the path of the pyramidal columns, as now held, are not to be admitted. He cut the left half of the pons Varolii, and found that the right half of the body had increased in power, and that the left had lost power; on one side dynamogeny, on the other side inhibition. He then made a section of the left half of the cord below the medulla, and found that the left half of the body, primarily enfeebled, became stronger in a state of dynamogeny. A median vertical section of the pons and medulla oblongata caused no paralysis. The irritation of a motor centre, after a hemisection of the spinal cord on the opposite side, caused a bipedic, diagonal movement; that is to say, sometimes a simultaneous movement of the right arm and the left leg, sometimes of the left arm and the right leg.—*Le progrès médical*, No. 17, 1882. Isaac Ott, M.D.

c—GENERAL PATHOLOGY OF THE NERVOUS SYSTEM.

To What Extent is Syphilis the Cause of Tabes, and What are the Results of an Anti-Syphilitic Treatment of the Latter ?—Prof. Leyden, *Berlin. Zeitschrift für klin. Med.*, iv, p. 475. The author expresses his views as follow : "Although I willingly acknowledge that the theory of syphilitic tabes numbers among its adherents names of good scientific repute, yet I must declare most decidedly against this theory and refuse to recognize an etiological connection between syphilis and tabes." He then reviews the opinions of others on this subject, referring particularly to Erb's contributions. After calling attention to the fact that Erb is not a dualist respecting syphilis, and pointing out the effect of this on his statistics, upon which his whole argument rests, he says : "Statistics in medicine are valued highly by many. I am not a great respecter of them. I claim distinctly that upon statistics alone no proof can be founded. They may support other evidence, but are not alone capable of furnishing an etiological demonstration. All considered, I am forced to the conclusion that the statistical relations of this question prove absolutely nothing. On this ground alone, however, is founded the entire theory of syphilitic tabes. * * * Respecting treatment, for my part I find that the anti-syphilitic treatment supplies nothing * * * except that I know of examples where repeated mercurial inunctions have reduced tabetic patients so low that the injury was irreparable. * * * The last point is one which Lancereaux in particular has emphasized ; namely, that the pathological anatomy of tabes is entirely different from that of the syphilitic process. This would be simply false if, as claimed, syphilis frequently transmitted diffuse changes in the substance of the central nervous system. The changes which syphilis produces are of a local nature. Accounts of the anatomical changes in syphilis of the spinal cord are not numerous in literature, being absent on account of the rarity of such affections, and the greater rarity of autopsies. We are better acquainted with syphilitic disease of the brain, and we are justified in drawing a certain parallel between the brain and the cord. In the former we recognize as syphilitic processes, circumscribed gummatous meningitis, syphilomata, and, finally, softening, which is to be attributed to syphilitic arterial disease. Of these changes we have, though rarely, analogous conditions in the cord. Circumscribed meningitis, particularly in the cervical region, has been frequently described. Wagner observed a case of syphiloma. The arterial disease has been recently investigated by Prof. Baumgarten, and a correspondence found between the brain and cord. Circumscribed myelitic softening as a result of syphilis has also been observed. I have myself described such a case (*Charite-Annalen*). Sclerosis as a result of syphilis may be spoken of, but only as circumscribed foci, as are all these lesions, which are, in the main, entirely different from that form to which tabes belongs. If, finally, it be asked, under what symptoms does syphilis of the cord develop, the reply would be, according to my experience, always with the signs of a circumscribed myelitis or myelo-meningetic affection. I have seen a not inconsiderable number of such cases involving one extremity, though, as a rule, both, and presenting, besides paresis, symptoms of rigidity, more or less marked. The syphilitic processes are established more frequently in the medulla oblongata than in the cord itself, as apoplectiform or as subacute bulbar paralysis. For these cases foci of softening have been demonstrated in the substance of the medulla and pons, in connection with disease of the arteries (branches of the basilar). In the extraordinarily severe cases of this kind obliteration of the basilar artery occurs (Griesinger, one case ; Eichhorst, one case ; Leyden, two cases). These rare cases of obliteration of the basilar artery appear to depend upon syphilis.

ON THE RELATIONS BETWEEN SYPHILIS AND TABES.—Dr. Pusinelli, Leipsic, *Arch. f. Psych. u. N.*, xii, 3.

He found in 51 cases of tabes that 16 were affected with, or gave a history of, syphilis (16 per cent.). He concludes, from the irregular relations existing between the two diseases respecting succession in time and the severity of the phenomena, that there is the possibility of an alternating action dependent on both diseases, in which, on the one hand, with an existing pre-disposition to tabes, syphilis would act as an additional factor in the development of tabes, while, on the other hand, the saturation of the system with syphilitic poison may act as a predisposing cause of tabes, particularly when added to other special causes of tabes. The question whether there is a true degeneration of the posterior columns dependent on syphilis has not yet been answered by the facts of pathological anatomy. A connection between the two diseases is not indicated in the result of treatment in his cases, for anti-syphilitic measures produced uncertain effects, and in one case mercurial inunction made the patient worse. He reaches the conclusion that all proof of a connection between tabes and syphilis is still absent, though a suspicion of such a connection is aroused by statistical facts.

THE ASSOCIATION OF TABES DORSALIS WITH SYPHILIS.—Dr. Thomas Buzzard, London.

Tabes dorsalis is the most frequent of all the chronic affections of the cerebro-spinal axis, and the prognosis in this disease is practically hopeless. There is increasing evidence, no doubt, to show that cases may go on for a very long time without the symptoms becoming materially intensified, and even with encouraging periods of improvement. But, as matters stand, we are unable to count upon more than this in any case of a confirmed character. At first sight, therefore, it would seem to be a point of more than ordinary importance, to discover whether the disease is so far connected with a syphilitic origin as to encourage us to hope for success by treating the patient energetically with specific remedies.

In many morbid conditions, such a discovery, by leading to appropriate treatment, produces the most triumphant results. But, as a matter of fact, in the case of tabes dorsalis, expectations that may have been formed of a similar success have not been realized. Now and then, it is true, we meet with cases which improve remarkably for a time while iodide of potassium is being administered, but in my experience I have never known a cure to result from specific measures. From the mercurial treatment, indeed, which I have had very carefully applied by inunction in a number of cases where the history of syphilis was distinct, I have seen no good whatever, but, on the contrary, as it seemed to me, a tendency to harm.

In 1871, writing upon the subject of syphilitic affections of the nervous system, I included locomotor ataxia among the nervous affections belonging to the tertiary stage of syphilis. At that time the frequency with which a syphilitic history was to be noted in these cases had long impressed me, and I was in the habit of treating my ataxic patients with iodide. But the remarkable absence of successful results appeared to throw so much doubt upon the matter that I carefully excluded the disease from consideration in my work on syphilitic nervous affections. The connection between nervous disorder and syphilis was then not generally recognized, and I was anxious to avoid weakening the force of that which was to be said on a very important subject by the introduction of debatable material.

I do not know that I am disposed to draw a very hard and fast line between tabes supposed to be of primary origin and that which is secondary to acute changes. It is certain that in inquiring into the history of many cases which, when they come before us, present all the characters of typical tabes dorsalis, we frequently hear a description of symptoms, often long past, which can only be referred to the occurrence of spinal meningitis, usually of a slight and strictly localized character. The fact that some slight thickening of the soft membrane and adhesion to the posterior surface of the cord is commonly met with in *post-mortem* examinations of tabetic cases is well recognized. Vulpian, who was at first disposed to think that this meningitis might be primary, has rejected the idea, because the change is not proportionate to the amount of lesion in the posterior columns, and he now attributes it to a propagation into the membrane of the irritative action going on in the neuroglia. But it is not disproved that the initiatory inflammation of the pia mater might spread itself by continuity to the neuroglia. There may possibly be anatomical reasons connected with the blood-vessels, which would explain the confinement of the inflammation which has thus been started, to a certain locality of the cord. I am inclined to think, also, from certain observations, that a limited spinal meningitis may clear off without leaving traces perceptible to the naked eye in the portion of membrane which has been affected.

The hypothesis which is admitted by Vulpian as the most probable, is that in the sclerosis of tabes it is the nervous element proper, contained in the nerve-tubes, which is first affected by an irritative lesion, and that the connective tissue suffers secondarily. The difficulty of accepting this view is, to my mind, very great. One cannot conceive an atrophy of axis-cylinders without some antecedent changes in the carriers of trophic material for the axis-cylinders, *i. e.*, in the blood-vessels, and the tissue in which they lie. On the other hand, if a vascular change be the initiatory step, it does not seem difficult to understand that syphilis, which is prone to occasion meningitis, may sometimes lead to sclerosis, through inflammation of the soft membrane of the spinal cord.

If we take the statistics of Fournier, Erb, and myself we shall find in 59.0 per cent. there was a history of syphilis. It is certain that coincidence is not sufficient to establish any thing like a necessary relation. It is necessary also to bear in mind another very possible source of fallacy. There is often great difficulty in ascertaining the earliest symptoms of tabes. There may have been some slight flying pains which have left little or no mark in the recollection of the patient, and in nine cases out of ten they have been set down by him as rheumatism. Yet these pains, usually the earliest evidence of tabes, may have occurred *before* he became affected with syphilis.

The position of tabes in regard to syphilis is peculiar in another respect. Affections of the nervous system which owe their origin to syphilis are not, as such, distinguishable in any very evident manner from diseases unconnected with such affections. As a matter of experience it is certain that almost all cases of marked paralysis of the cranial nerves (I exclude here the incomplete and transitory paralysis seen in tabes) are due to syphilis.

If tabes be of syphilitic origin, how is it that females, who have their fair share of other diseases of the nervous system of specific origin, furnish only ten per cent. to the ranks of the former disease?

Whilst it appears to me incontestable that there is a remarkable frequency of association between syphilis and tabes dorsalis, I do not think, all things considered, that the time has yet arrived for us to draw safe inferences as to the precise nature of the relation. —*Lond. Lancet,* June 10, 1882, 391.

CEREBRO-SPINAL MENINGITIS IN A NEW-BORN INFANT.—Bambas. A young woman became affected with cerebro-spinal meningitis at the end of her pregnancy. She died after having given birth to an apparently healthy child, which presented, two hours later, symptoms of meningitis, followed rapidly by death.—*Progrès médical,* No. 17, 1882.

SYPHILIS OF THE SPINAL CORD.—D. F. Greiff, Heidelberg. *Arch. f. Psych. und N.*, xii, 3. The author calls attention to the imperfection of our knowledge on this subject, compared with that of cerebral syphilis. The anatomical changes found in the latter, consist of specific neoplasms, either as circumscribed gummata, or diffused gummatous infiltrations; specific inflammatory processes of the meninges and adjacent cerebral substance; and, finally, syphilitic disease of the cerebral arteries and its effects, first described by Heubner. Reviewing the literature of syphilis of the cord, he finds: (1) cases of circumscribed gummata, in some originating from the envelopes of the cord, and in others developing within the cord itself (cases of Rosenthal, MacDowell, Willis, Wagner, Hales). (2) Diffused neoplasms (cases of Zambaco, Bruberger, Westphal, Heubner). (3) Inflammatory changes in the meninges associated with further changes in the cord itself (cases of Homolle, Winge, Charcot, and Gombault, Schultze, Julliard). No description is to be found of specific diseases of the vessels of the spinal cord. The dilated vessels, with thickened walls, surrounded with cellular infiltration, described in some cases, as they appear under other circumstances also, cannot be acknowledged as specific. The same doubt must remain in all cases where inflammatory processes of the cord and membranes are unaccompanied by specific gummatous tissue, and notwithstanding many attempts, the problem of determining the characteristic anatomical relations of syphilitic myelitis remains, he thinks, unsolved. He gives an abstract of 13 cases with autopsies, and refers in detail to Juillard's views, who considers that the combined presence of inflammatory processes in the meninges, of exudative processes in the vessels and their sheaths, and then hyperplasia of the neuroglia with its effects upon the neural elements, constitute the characteristics of syphilis of the spinal cord. As the pathological processes involve principally the lymphatic system of the cord, entering through the meninges, the neuroglia, and the vascular sheaths, it results that the changes may be diffuse, but they cannot constitute a "systematic disease. If the process be a rapid one, softening follows; if slow, sclerosis. The author reports very fully his own case, in which the changes consisted of an extensive inflammation of the pia—in some portions just beginning, in others at an advanced stage; decided disease of the arteries and veins, also a swelling and hyperplasia of the interstitial tissue, with inflammatory exudations around the vessels, and moderate involvement of the neural elements. The arterial changes were the same as those described by Huebner for the brain, observed in the cord for the first time. A peculiar obliteration of the veins also existed. No softening or decided "systematic" lesion was found. He concludes that the facts of this case support Juillard's views, and furnish undoubted proof of the existence of a syphilitic disease of the arteries of the cord, but while the changes in the meninges and the vessels appear to be undoubtedly of a specific character, the changes in the cord, on the contrary, exist only in connection with and dependent upon the former, which together represent the true character of syphilis of the spinal cord.

PHENOMENA PRODUCED BY THE APPLICATION OF A GALVANIC CURRENT TO THE HEAD OF AN HYPNOTIZED HYSTERICAL SUBJECT, DURING THE LETHARGIC PERIOD.—*Progr. méd.*, 1882, 2. Charcot found that in this state an increased electro-muscular excitability existed, corresponding to the mechanical excitability of such cases. Opening and closing of the constant current (4 to 10, Leclanché), with the anode on the frontal or parietal region of the head and the cathode on the sternum, caused contractions, on the side opposed to the anode, in the face and extremities. It did not produce waking, and the same procedure produced no effect when the subject was awake.—*Deutsch. med. Woch.*, No. 21.

ON THE FAILURE OF THE KNEE-PHENOMENON.—Carl Hertzka. The author recognizes partially the diagnostic importance of the failure of the patellar tendon reflex, but advises caution respecting its pathognomonic value. He examined a patient with symptoms of tabes, in whom the patellar tendon reflex was absent, who, nevertheless, was a contortionist. This symptom is absent in children also, although they are not tabetic; here and there, however, other neuropathic tendencies are observed. The author believes that Westphal's view—that the knee-phenomenon only disappears when the outer portion of the posterior column in the lumbar region becomes involved, and remains preserved by other localizations of the disease—is the most probable one. In post-diphtheritic ataxia, this symptom disappears, to return again after recovery from the disease. He does not accept Bergius' application—" pseudo-tabes"—for such cases. He found in many individuals with absent knee-phenomenon, a high degree of abdominal plethora. An etiological factor, in that hyperæmia of the lumbar region induced by this means, may simulate the initial symptoms of tabes. Mechanical conditions also arrest or hinder the execution of the knee-phenomenon, as thick cushions of fat, short tendons, interference with the vibratory power, and diminution of the muscular tone.—*Centralblatt f. Nerv., etc.*, June, 1882, from *Pester med.-chirurg. Presse*, No. 11, 1882.

ON REFLEXES AND PSEUDO-REFLEXES.—A. De Watteville. Although pertaining more to physiology than to pathology, we present an abstract of this author's paper as an accompaniment to Wesphal's observations cited below.

" The question is still debated whether muscular contraction following the percussion of a tendon is a true reflex phenomenon or not. It is agreed on all sides that it ultimately depends upon a reflex action, since it is abolished by the destruction of the posterior roots [see Westphal's experiments in opposition to this statement. W. R. B.]. Those who do not consider the contraction to be a direct reflex, hold that its cause is the excitation of the muscular substance itself by the sudden stretching; and explain the integrity of the reflex loop necessary for its occurrence, by assuming that the excitability of muscle to this particular stimulus depends, like tonicity, upon a reflex influence from the cord, or rather a manifestation of this tonicity. The chief reason for rejecting the direct reflex theory is that the time which elapses between the instant of percussion and that of contraction is too short to admit of a centripetal stimulus being converted in the cord into a motor impulse, on the usually adopted measurements of the rate of nervous vibration. Now measurements of the period of latent contraction following the percussion of a tendon, and of the rate of nerve vibration, present too wide discrepancies, as given by different authors, to give very accurate data for calculation ; and the process itself of calculation from one set of data to another, opens the door to too many fallacies. In order, therefore, to avoid the uncertainties attending such a method of proof, it has seemed to me desirable to obtain myographic records of the knee-jerk and the plantar reflex on the same subject and with the same method. In this way the results are at least comparable. The absolute measurement may not be correct, but, if so, the error is the same in both series of results, and thereby eliminated." He presents a tracing of the result of his experiments, after the method described by Waller ("Tendon Reflex," *Brain*, 1880), and reaches the following conclusions : "1. The latency of the plantar reflex is about three times as long as that of the so-called 'tendon reflex.' The latter, in my tracings, has the usual duration ascribed to it by most authors, viz. : about 0.3 of a second. 2. The muscular contraction following cutaneous stimulation tends to run a more protracted course than that following the percussion of the tendon. 3. In certain cases, percussion of the tendon is followed by a contraction, of which the latency and the protraction correspond exactly to those characteristic of the reflex contraction occurring later, and running a more protracted course than that due to the electric stimulus. 4. In comparing the latencies of contraction of the extensor femoris and of the gastrocnemius, following excitations at the sole of the foot, it must be borne in mind that the difference will be comparatively small, owing to the fact that the two muscles receive their nervous supply near the knee. The difference likewise between the knee-jerks following cutaneous excitation at the sole of the foot and at the patellar tendon respectively, will be equal to the time taken by the transmission of the stimulus from the sole to the knee. 5. The possibility of true reflex contractions following a galvanic shock or a tap on the tendon is one of great importance in diagnosis, and deserves further consideration and elucidation.

"I have investigated myographically the question of 'crossed tendon reflex.' The tracings show that the latency of contraction in the opposite leg is absolutely identical with that in the leg percussed—a fact in itself sufficient to disprove the reflex nature of the phenomenon. Further, it is easily shown that a blow given over the tibia or patella of one leg with the fleshy part of the hand, or, still better, on the heel of the fully extended leg, produces little or no contraction in that leg, whilst the opposite is *adducted* vigorously ; a blow given on the heel of the right bent leg causes no contraction in the opposite limb. It is plain, therefore, that the shock conveyed along the femur to the pelvis, and acting mechanically on the most favorably placed tendons, is quite sufficient to explain the facts—a view obviously forced upon us by the results of Prevost and Waller's experiments on rabbits. In conclusion, I wish to advert to one difficulty attending the 'direct muscular' or 'myotatic' theory of the knee-jerk and allied phenomena. Though its latency is too short to justify the reflex hypothesis, is it not too long to be readily reconciled with the accepted periods of latency to electrical and other direct stimuli ? We know, on the other hand, that the time of latent stimulation of nerve-centres (cortical substance : Franck and Pitres, Richet, Bubnoff, and Heidenhain) is much longer than of peripheral organs ; while we have in the muscles themselves organs (end plates) which stand, so to speak, midway in some of their physiological properties between a nerve cell and a nerve fibre. The solution of that difficulty (if it is one) might, therefore, be found in the assumption that the excitation produced by the sudden stretching of a muscular fibre falls primarily upon its nerve-ending. None of the known facts, at least, militates against this view, which I throw out more as a 'working hypothesis' for future investigation, than as a scientific explanation of the phenomena."—*Brit. Med. Jour.*, May 25th.

ON A SOURCE OF ERROR IN THE EXAMINATION OF THE KNEE-PHENOMENON, etc.—Prof. C. Westphal, *Arch. f. Psych. und Nerv.*, Bd. xii, 3, 298.

The author calls attention to the fact that certain patients, in whom the knee-phenomenon has been positively absent for a long period, may exhibit at certain stages of the disease strong contraction of the quadriceps on percussing the patellar tendon.

Three cases are cited in which this occurred, persisting for a short time, and then disappearing. The author concludes that this is not a return of the tendon-phenomena, but is a skin reflex ; for when a fold of the skin over the patellar tendon was lifted and squeezed between the fingers, contraction of the quadriceps followed ; also pinching and percussing a fold of the skin in other parts of the leg may excite in some cases contraction limited to the quadriceps or extending to other muscles of the leg and thigh. There exists, he says, two independent phenomena : namely, a true, and a pseudo knee-phenomenon. It may be claimed that, as in tapping over the tendons, the skin is necessarily excited ; at the same time the presence of the true phenomenon in such a case could not be proven. Opposed to this are the facts that, in the first case (tendon-phenomena), the ordinary immediate and quick contraction follows percussion, while in the second (skin reflex), at least at first, a distinctly appreciable interval occurs. As a rule, also, stronger percussion is required to produce contraction in the latter than in the former case.

The author reports experiments made by himself and others to elucidate this nature of the tendon-phenomena ; in one of which, notwithstanding that sections of the fifth, sixth, and seventh lumbar, and first sacral pairs of nerves were made, the knee-phenomenon remained completely preserved. He enters into a consideration of the question whether it is a true reflex or not. Although leaving the question undecided he still favors his former view, that muscular tone and a certain degree of tension and vibratory power of the tendons are the only conditions necessary for the production of the tendon-phenomenon, the additional theory of a reflex process by means of centripetal nerves from tendons or muscles being unnecessary.

ON THE MUSCULAR ATROPHY FOLLOWING CERTAIN ARTICULAR LESIONS.—Prof. Charcot. *Progrès méd.*, May 20, 1882.

The author in considering this question says, that the clinician must be on his guard in the presence of even a very slight inflammation of a joint, it may disappear very quickly, and yet consecutive atrophy of the muscles may last for a very long time. *There is no necessary relation between the intensity of the articular affection and that of the paralytic and atrophic phenomena.* It is the rule for the paralysis to persist after the arthritis has ceased. In such cases a more or less direct relation must exist between the cells of origin of the centripetal nerves (articular), and the cells of origin of the motor and trophic nerves of the extensors. From a physiological stand-point, it is a deuteropathic affection of the cord. The articular disease having, by means of the irritated articular nerves, reacted on the spinal centres, modifying those centres from which depart the motor nerves, and those which preside over the nutrition of the muscles. These centres do not undergo a profound modification, the process simply consisting of a sort of inertia or stupor of the cellular neural elements. The treatment consists in the application of electricity in any of its forms.

W. R. Birdsall, M. D.

d.—MENTAL PATHOLOGY.

Criminal Lunatics.—Dr. Ferrier (*Brain*, April, 1882) calls attention to some peculiarities of the brain of a woman who had murdered her two children, and was in consequence confined in the Broadmoor Asylum. The case was not, strictly speaking, that of an insane criminal, and the cerebral changes, which were not teratological, were of interest only from their bearing on the localization theories of Ferrier. Dr. Charles K. Mills (*Medical and Surgical Reporter*, May 13, 1881) discusses the general subject of insane criminals incidentally, alluding to the results of investigations into the cerebral anatomy made by Benedikt, Osler, and himself. In the course of the paper he approvingly cites the conclusions of Lacassagne, Lebon, Kräpelin, and Ferri, mentioned in the January number of this Journal, page 221.

The Insane on Criminal Responsibility.—There was a discussion in the Hanwell Lunatic Asylum Debating Society, which consists of the patients of the asylum; on the question of the responsibility of the insane for murder. The view was pretty generally taken by the patients that the outside world was not mentally sound on this subject, and that insanity ought never to be a defence, because it is never so complete as to extinguish all sense of responsibility. To test responsibility, it was proposed to offer the lunatic committing a criminal act a red-hot poker, and by his grasping or not grasping it to determine his responsibility. One of the patients, who shot at the Queen, admitted that if the lunatic who had previously attempted the crime had been executed he would not have attempted it. During the Guiteau trial a lunatic in the New York City Asylum, who presented the same type of insanity as the assassin, whose case has already been partially described (Journal of Nervous and Mental Disease, vol. vii,